

LETTERS TO THE EDITOR.

The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

The Intake of Carbon Dioxide—a Correction.

WILL you give me the opportunity of making the following correction in my Presidential Address to the Chemical Section of the British Association.

I stated incidentally that Mr. F. F. Blackman, in his well-known experiments on the intake of carbon dioxide into the two sides of an assimilating leaf, employed air enriched with that gas to the extent of 4 per cent. and upwards.

Mr. Blackman has pointed out to me that in the experiments in question he used air containing only from 1·8 to ·33 per cent., and that since the publication of his earlier results he has still further reduced this amount. In fact he is of the opinion that his method is applicable to the measurement of the intake of carbon dioxide of even a much greater degree of dilution.

The error was an inexcusable one on my part, but does not affect the main argument that the natural rate of intake cannot be directly deduced from experiments in which the carbon dioxide content of the air materially departs from the normal amount 0·03 per cent.

HORACE T. BROWN.

52 Nevern Square, Kensington, S.W., September.

Geological Time.

IN his Presidential Address to Section C at Dover, Sir A. Geikie has offered a bold challenge to Lord Kelvin and those who agree with him by calling upon them to give due weight to geological phenomena in forming an estimate of geological time. Permit me to say what I think about it.

It seems to me probable that, when the grand idea of the universal dissipation of energy had occurred to Lord Kelvin, he saw that the principle must be applicable to the earth, and that, if the law of conduction of heat could be used, he might from it obtain an estimate of the world's age. He then instituted his important experiments to determine the conductivity of rocks *in situ*, and found the value 400 (more or less), the units being a foot a year, and a degree Fahr. But it was necessary, for his calculation to succeed, that he should assume the earth to be solid. If I do not misjudge him, I think he then sought for arguments to prove this point. Now, I cannot but think that the proofs of solidity on which physicists rely are by no means convincing; and if the earth is not solid, Lord Kelvin's estimates are without foundation. Moreover, it is not sufficient that the earth should be solid at the present, to which these proofs refer, but it needs to have been so from the beginning of the time to which his estimates go back.

Prof. G. Darwin, in his book on the tides (p. 237), has done me the honour of referring to my "Physics of the Earth's Crust" as if I am an arch-heretic on this question of solidity. Whether my arguments are beneath notice, or whether there is a difficulty in answering them, I do not know; but they have never been refuted, while they are held to be of decided force by some geologists, and among these by the Indian Geological Survey.

Harlton, Cambridge, September 25.

O. FISHER.

The Terrestrial Gegenschein.

I DO not know whether the phenomenon I am about to describe has ever been noticed. The circumstances under which it is noticeable must occur rarely. They are these:—

I spent some time of the summer of 1898 on an isolated mountain peak, surrounded by lower mountains whose sides were densely wooded. The result was that near the time of sunset the shadow of my own mountain peak was visible on a mountain side which might have been three or four miles distant. One evening I amused myself by watching the shadow of the peak as the sun was descending. My attention was attracted by an illumination in the direction opposite the sun so strikingly resembling the astronomical gegenschein, that at the first glance I saw in it an explanation of the latter. It consisted of a somewhat bright glow, which might be a degree or two in diameter, but which shaded off by such imperceptible gradations that a definite extent could not be assigned. A

little study, however, showed an explanation. As I have said, the mountain on which the glow was seen was densely wooded. In such a case the shadows of those leaves and branches which the sun's rays first reached fell upon the interior foliage and obscured it. But an observer looking from the exact direction of the sun will see through the foliage as far as the sun's rays extend. In other words, the visible surface on which he is looking will be entirely illuminated by the sun's rays, whether this surface is formed of the outer strata of foliage or of a strata ever so far inside, which can be seen only through the crevices in the outer stratum. The shaded interior will be entirely invisible to him. But if his point of view is in a direction ever so little oblique, he will see only the outer foliage illuminated, while more or less of the interior foliage which he sees will be in the shadow. Thus the region exactly opposite the sun will be seen in its full brilliancy, while the neighbouring region will be a mixture of light and darkness. At a distance of several miles this compound of light and darkness will be fused into a single half-shade, strongly contrasting with the full brilliant light of the opposite point.

It is clear enough that we cannot have such a state of things as this in the case of the astronomical phenomena. Yet the phenomenon seems to be of sufficient interest to warrant its being placed on record.

S. NEWCOMB.

The Cause of Undercurrents.

IN NATURE of August 3, p. 316, is given a letter from Rear-Admiral Sir William Wharton, in which he states that he is diametrically opposed to my opinion about the double currents in the Straits. He says that "Admiral Makaroff considers that difference of density of the water is the primary, and, indeed I gather he thinks, the only cause of these opposing currents; but he brings no evidence beyond theoretical considerations in support of his belief"; further, in his letter, Admiral Wharton refers particularly to the double current of the Bosphorus, of which I spoke in my lecture at the Royal Society of Edinburgh. I cannot leave unnoticed remarks from so distinguished a hydrographer, who, during his long work, has contributed so much to the advance of science. My researches about the Bosphorus are published only in Russian, in a book named "On exchange of water between Black Sea and Mediterranean" (St. Petersburg, 1885). Should Admiral Wharton know my language, he would easily come to the conclusion that my opinion about double currents in the Bosphorus are based upon the observations made in 1881 and 1882. I then invented an instrument for measuring the current at different depths, and gave the name of "fluctometer" to it. The instrument consists of a propeller revolving on a horizontal spindle. A bell is attached to the propeller, and at every revolution of the propeller it strikes twice. As water is a very good conductor of sound, the number of revolutions could be counted through the bottom of the ship (provided the ship is not sheathed with wood) at all depths to which the instrument was lowered (40 fathoms). I used to anchor in the middle of the Bosphorus for a couple of days at a time, and make a series of observations every two hours. In order to obtain more detailed data, I used to take the samples of water from the same depth to which the fluctometer was lowered. Twice I used to go along the Bosphorus from the Black Sea to the Marmora Sea in order to learn in what depth is the limit of two currents. In volume xxii. of the *Proceedings* of the Royal Society of Edinburgh, Plate I. shows a position of the limit of two currents, mean velocity of both currents, and specific gravity of water. In Plate II. is given a sketch of my "fluctometer."

I am sorry that the limits of this paper do not allow me to give particulars of my observations, but I believe some of my deductions, worked out from direct observations, would be interesting to English readers.

Mean velocity of the upper current, $3\frac{1}{4}$ feet per second. It varies from 0 to 10 feet per second in certain places.

Velocity of the upper current diminishes with every fathom of depth.

Limit between two currents close to the Marmora Sea is at 11 fathoms. It gradually goes down to 27 fathoms close to the Black Sea. Limit between two currents is influenced by winds and by barometrical pressure, but not very much.

Lower current has close resemblance with the river. Its velocity does not vary very much. We never found anywhere

lower current less than 1'84 and more than 3'22 feet per second. Mean velocity of the under current was 2'32.

The upper current does not everywhere occupy the full breadth of the strait. It flows in some places under the north coast of the strait, and in other places under the south coast. Lower current also does not flow over the whole breadth, and occupies a certain part of the bed of the strait. In my book I give a chart of the Bosphorus, where I show direction of the upper and lower currents. A glance at that chart will show that there are places where both currents can be found, and there are places where the instrument will show the existence of only one of them, and in some places the explorer will not find either upper direct current or under current.

Difference of level of Black and Marmora Seas, calculated from difference of specific gravity of water, is 1 foot 5 inches.

Mean specific gravity of water ($\frac{17.5}{517.5}$) entering from the Black Sea into the Bosphorus is 1'0140. Mean specific gravity of water entering from the Marmora into the Bosphorus is 1'0283.

By upper current pass 370,000 cubic feet per second. By lower current pass 200,000 feet per second. Difference between these two figures being 170,000 feet per second is due to the excess of fresh water in the Black Sea.

I hope that after reading these figures and deductions Admiral Wharton will change his opinion, and come to the conclusion that my idea about double current in the Bosphorus is based, not only on theoretical considerations, but also upon direct measurement.

Admiral Wharton expresses also his opinion about double current of the strait of Bab-el-mandeb, and says that "there are none of the differences of specific gravity." I may be permitted to refer to my other book, "*Le Vitiâz et l'Océan Pacifique*," where I give (p. 136, plate xxvii.) specific gravity of the strait of Bab-el-Mandeb measured by myself. Examination of figures given there by me shows that there is a difference of specific gravity which produces double current in that strait.

Nobody can deny that the wind has a great influence upon the movement of surface water; but I hope that Admiral Wharton will agree with me that differences of specific gravity has also some influence upon the circulation of water in the seas generally and in the straits particularly. S. MAKAROFF.

Ermack, Newcastle-upon-Tyne, September 23.

Movement of Sea-Gulls with a Coming Change of Weather.

THE suggestion that sea-gulls may have some meteorological sense would be best tested by inquiry as to whether there was, at the time of their westward flights, observed by your recent correspondents, any other possible motive for their journeys; and particularly what food was available in the Channel at the time.

Neither of your correspondents needs to be told that every sea-gull is a semaphore to every other sea-gull in sight of him, nor that one gull "working on" fish will presently be surrounded by others from all sides.

In clear, fine weather the news of abundant sprats would be passed along the Channel in this way, faster than by military signallers, and answered by a concentration of gulls much speedier than any possible to troops.

Moreover these birds, and vultures, as I know from repeated observation, do not merely follow each other round the headlands. If a gull in a bay sees another gull hurrying from the offing into the next bay, he does not fly round the headland between, but rises over it, well knowing that from the upper air he will see whatever hurried the outer gull. As he does so his motions will be observed, and probably acted on by others further within the first bay; and if food be failing in the Thames, and abundant off the Wight, there will be plenty of gulls flying across Kent, and some across even Surrey. Here in Chelsea I seldom see large gulls on the river. But it is a common enough thing to see them flying high overhead to the south-westward.

Westerly and south-westerly gales are so common in the Channel that neither beast nor bird can make any movement without a good off-chance of finding one on his way. It has to be remembered that most of our migratory sea fish are apt to run up Channel in the warm half of the year, so that the message, "Plenty of fish on the surface," is probably most often passed from west to east.

We shall, I think, need a good many simultaneous observations at various points and of various matters before we make it even probable that sea-gulls can foretell a south-west wind, and will then go to meet it. They cannot eat it; and, if strong, it will give them little leave to eat anything else; so the motive is not apparent.

W. F. SINCLAIR.

102 Cheyne Walk, Chelsea, S.W., September 22.

On the Use of the Fahrenheit Scale for Observations on Sea Temperatures.

IN addition to the Fahrenheit scale being so much more practical for observation in meteorology than the Celsius, allow me to point out that in observations for ocean temperature it is even more so, and especially when we come to deal with observations taken in the polar regions. Here with the Fahrenheit scale we have never to deal with a minus reading at all; whereas with the Celsius scale it is a constant change from plus to minus and minus to plus. This introduces a source of very serious error both in observation and calculation, besides adding to the work, and therefore the cost of working up results. The boon of never having to think of a minus in such work is not to be lost sight of for the sake of fashion. As one who has taken part in extensive observation and calculation work at Ben Nevis Observatory, on board ship, and in connection with the Scottish Fishery Board, I would also urge the use of the Fahrenheit scale for meteorological observations on the same grounds as Mr. J. Y. Buchanan and Mr. H. Helm Clayton.

Joppa, Edinburgh, September 25. WILLIAM S. BRUCE.

Cave Shelters and the Aborigines of Tasmania.

I HAVE just received news from Mr. J. B. Walker, of Hobart, of the discovery of some interesting relics of the aborigines of Tasmania. Mr. Walker accompanied Mr. R. M. Johnston, the Government Geologist, on an expedition in search of some remains of Tasmanians, and the party were rewarded by finding a hitherto unknown so-called quarry where the natives manufactured some stone implements, also a cave which showed considerable evidence of having been used by the aborigines, as well as a tree notched by them for climbing purposes. The sandstone cave or rock shelter is situated in Hutton Park, near Lovely Banks. The quarry is situated at Coal Hill, two miles north of Melton Mowbray, about 40 miles N.N.W. of Hobart, and 1100 feet above the sea-level.

The discovery of this quarry makes the tenth known quarry used by the aborigines, and the first mention of their use of cave or rock shelters.

H. LING ROTH.

Halifax (Yorks.), September 27.

The Darjeeling Disaster.

UNUSUALLY large seismograms were obtained in the Isle of Wight on September 3, 10, 17, 20 and 23. The first three refer to disturbances originating in Alaska. The fourth refers to disasters in Asia Minor, and the last to an earthquake having an origin as distant as Japan. Since the 23rd in the Isle of Wight, and I believe also at Kew, not the slightest movement has been recorded. The inference is that the great earthquakes reported as having taken place at Darjeeling on the night of September 25-26 are at the most small and local, and are not likely to have been recorded outside the Indian Peninsula. It is extremely likely that the tremors noticed in Darjeeling were due to landslides, and seismic phenomena were entirely absent.

J. MILNE.

Shide Hill House, Shide, Newport, Isle of Wight, September 27.

Lectures at the Royal Victoria Hall.

I SEE in your issue of September 21 (p. 513) the statement that I am to lecture at the Royal Victoria Hall on "Photographs taken in the dark." I beg to say that the title I gave for my lecture was "Pictures taken on a photographic plate in the dark." I suppose the authorities at the Hall consider the titles identical. I do not.

W. J. RUSSELL.

St. Ives, Ringwood, Hants, September 26.